motion is opposed by an internal resistance or friction which is measured by the logarithmic decrement and is distinct from the elastic after-effect. In the new edition six pages are devoted to Boltzmann's theory of internal friction of solid bodies, according to which the damping of oscillations is regarded as due to the elastic after-effect, the result of which is that the force urging the wire towards its position of equilibrium is not proportional to the displacement. In the fourth edition there was little about "Hardness," beyond a list of the substances forming Moh's scale; indeed, the whole article on the different kinds of *Festigkeit* was rather unsatisfactory. Now the researches of Hertz and the experiments of Auerbach have made it possible to measure the hardness of a substance absolutely and without reference to the properties of any other substance.

Instead of being relegated to the volume on heat, as in some text-books, the kinetic theory of gases is here described at considerable length in the section treating of the properties of gases. This section has been overhauled, and Stefan's theory of gaseous diffusion is given in place of O. E. Meyer's. Prof. Wüllner attaches so much importance to recent developments of the electromagnetic theory of light, that he has determined to alter the sequence of the subjects in his treatise, so that light and radiation may come at the end. In the edition now appearing, the second volume will therefore treat of heat, and the third of electricity. *pv.*

Peru. Beobachtungen und Studien über das Land und seine Bewohner während eines 25-jährigen Aufenthalts. Von E. W. Middendorf. (Berlin: Robert Oppenheim, 1893-94.)

DR. MIDDENDORF commences his treatise on Peru by a long preface detailing the circumstances which led him to choose that country for his home. Starting as the surgeon of an Australian emigrant ship from Hamburg in 1854, he narrates the incidents of the voyage, an epidemic of cholera and other irrelevant accidents included, until on the return journey, sick of the sea, he left his vessel in Valparaiso, and after some further wandering took up his residence in Peru, and was led by degrees to pay closer and closer attention to the land and people of his choice.

The work is planned in three parts. The first volume deals nominally with Lima, the capital of the republic, but is really much wider in its scope, commencing with the history of Peru, and after describing the town with its streets and public buildings in somewhat tedious detail, proceeding to discuss the general institutions of the country from the standpoint of the capital. People, church, government, law, education, commerce, transport, and charities are all discussed; and at the end two chapters describe the municipal markets, slaughter-houses, water-works, bull-fighting arenas, theatres, and other places of amusement. The second volume describes the coast lands of Peru northward and southward of the capital, the towns and seaports, the provinces now ceded to Chile, the railway communications, and the antiquities. A third volume is promised dealing with the highlands, but presumably the scheme does not include an account of trans-Andine Peru.

We cannot look on Dr. Middendorf's work as an exhaustive or even a comprehensive work on Peru. It is indeed a book for the general reader rather than the student. Abounding as it does in personal reminiscences, and written in an easy conversational style, it should do much to further the knowledge of the country it describes amongst German-speaking people. But it is, we fear, too diffuse and bulky to serve this purpose with the same degree of satisfaction that a smaller work might have secured, and it lacks firm and wide generalisations which could present a clear picture of the land and people as a whole.

LETTERS TO THE EDITOR.

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The Liquefaction of Gases.

A CAREFUL study of his letter in NATURE of February 14 shows that Prof. Dewar makes four claims.

(1) He claims that in his lecture in 1884 he employed an apparatus "more readily and quickly handled" than that of Messrs. Wroblewski and Olszewski, in the illustrations he gave of the work of these investigators.

(2) He claims that in his lecture in 1884 he used liquid nitrous oxide for the production of liquid oxygen.

(3) He claims that in 1886 he liquefied oxygen by passing the gas through a long copper coil surrounded by liquid ethylene, and that his apparatus made it possible to transfer the liquid oxygen to a glass vessel wherein it could be used as a cooling agent.

(4) He claims that, in conjunction with Prof. Liveing, he determined the refractive index of liquid oxygen before the publication of Prof. Olszewski's first paper on the subject.

I shall consider these claims in order.

(1) The apparatus used by Messrs. Wroblewski and Olszewski in 1883 is fully described and figured in *Wiedemann's Annalen* for that year (xx., 243). The gas to be liquefied was strongly compressed in a glass tube, imbedded in an iron cylinder, and connected by a capillary tube with a glass vessel, which in turn was connected with a store of liquid ethylene, previously cooled by passing through a narrow copper tube two metres long immersed in a bath of solid carbon dioxide; the glass vessel was also connected with a double oscillating Bianchi air-pump. The liquefied gas was collected in the glass vessel, and its properties were examined. A comparison of the apparatus of the Polish Professors with the brief description in the *Proceedings of the Royal Institution* of the apparatus used by Prof. Dewar in 1884 "for the purpose of lecture demonstration" will, I think, show that I stated the matter fairly in my former letter by saying that Prof. Dewar "describes and figures an apparatus which is a slightly modified form of that of the Polish Professors, which in turn was derived from the apparatus of Cailletet."

(2) If anything was gained by using liquid nitrous oxide for the liquefaction of oxygen, why has not Prof. Dewar continued to use this refrigerator?

Prof. Dewar claims to have devised a method for liquefying oxygen which was so much inferior to that brought into practice by Wroblewski and Olszewski that it was abandoned almost as soon as it was devised. I suppose his claim may be admitted here.

(3) Granting that the substitution of a long copper coil for a strong glass tube was an improvement, it must not be forgotten that the apparatus of Messrs. Wroblewski and Olszewski, described in 1883, enabled the liquefied gas to be collected in a vessel wherein its properties not only might be, but were, studied. That this apparatus was an efficient instrument is shown by the facts that before the year (1886) in which the copper coil method was described, Profs. Wroblewski and Olszewski had liquefied oxygen, nitrogen, and carbon monoxide: Prof. Olszewski had liquefied air, nitric oxide, and marsh gas; had determined the boiling points, at atmospheric pressure, and the critical temperatures and pressures, of these six gases; had solidified nitrogen, chlorine, hydrogen chloride, nitric oxide, carbon monoxide, and marsh gas; and had made careful determinations of the behaviour of hydrogen at - 211° and - 220°. But with his copper coil 45 feet long Prof. Dewar did nothing; for, so far as can be judged from his published papers, he did not initiate experiments on the properties of liquefied oxygen, nitrogen, or air, until 1891-92. I think we may conclude that the apparatus of the Polish Professors was better adapted for accurate work than "that made wholly of metal" to which Prof. Dewar refers. If Prof. Dewar's apparatus of 1886 was "safer and better" than "that used in Cracow in 1890"—as he says it was—why was not this safer and better apparatus turned to some scientific use before a description appeared of the apparatus of Prof. Olszewski, in which the gases were liquefied in a steel cylinder and then transferred to

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a glass vessel? In his letter of February 14, Prof. Dewar tells us that by means of the copper coil apparatus of 1886 "liquid oxygen was made and decanted or transferred from the vessel in which it was liquefied to another by means of a valve, and thereby rendered capable of use as a cooling agent." Why, then, did he not use the liquid oxygen as a cooling agent? It is strange that seven years after this, when Prof. Dewar wished to "prosecute research at temperatures approaching the zero of absolute temperature," he says he had "no recorded experience to guide [him] in conducting such investigations." Had he forgotten his own apparatus with its copper coil 45 feet long?

Prof. Dewar's apparatus of 1886 must have been peculiar; in one sentence in his letter of February 14, its deviser speaks of it as "entirely different in type from the crude plan Olszewski adopted in 1890," and in another sentence he assures us it was "practically identical in principle with that used in Cracow in 1890."

The apparatus of 1886 succeeded, in Prof. Dewar's hands, in producing solid oxygen; why have we heard nothing of that solid oxygen? Determinations of some of the constants of that substance would be interesting and important.

I must beg attention to an instance of Prof. Dewar's boldness. In 1883 Messrs. Wroblewski and Olszewski liquefied oxygen, and other gases, in a strong glass tube; in 1884 Prof. Dewar repeated some of the experiments of the Polish Professors, using, as they had done, a strong glass tube, but adapting some parts of their apparatus so as to make it "for the purpose of lecture demonstration . . . more readily and quickly handled"; in 1890 Prof. Olszewski described an improved and enlarged instrument, based on that described by his colleague and himself in 1883, wherein the liquefaction was effected in a steel cylinder. Yet, in his letter to NATURE of February 14, Prof. Dewar says "replace the glass tube in *my* apparatus of 1884 by a small steel cylinder, and attach to its lower end a narrow copper tube with a stopcock, and the Olszewski apparatus of 1890 is produced." (The italics are mine.)

(4) As regards Prof. Dewar's joint claim to priority in the determination of the refractive index of liquid oxygen, I admit he is right and I was wrong. I was misled by the date at the beginning of the paper which contains the earliest measurements by Messrs. Olszewski and Witkowski of the refractive index of liquid oxygen. The paper is headed *Extrait du Bulletin de l'Académie des Sciences de Cracovie, Octobre* 1891. At the end of the paper is the date 15 *Juillet* 1892; this, and the footnote, quoted by Prof. Dewar, make it evident that 1891 at the beginning of the paper is a printer's error for 1892. I am sorry I made this mistake. Nevertheless, considering that Olszewski's quantitative experiments on the optical properties of liquid oxygen were begun in 1887 (*Wied. Ann.* xxxiii., 570), and were continued in January 1891 (*ibid.* xlii., 633), I hold I was justified in referring to Prof. Dewar's experiments on the optical properties of liquid oxygen as "mainly repetitions of the work of Olszewski and Witkowski."

Towards the end of his letter Prof. Dewar refers to a quotation I made from one of his lectures ("Having no recorded experience to guide us . . ."); he follows my quotation (of which he gives only a part) by another to the effect that "the necessity of devising some new kind of vessel for storing and manipulating exceedingly volatile fluids, like liquid oxygen and liquid air, became apparent when the optical properties of the bodies came under examination." I had expressed admiration "of the skill which produced" Prof. Dewar's new vacuum receivers. Prof. Dewar now tells us that when he prefaced his account of the making of these receivers by the sweeping assertion that he had no experience to guide him in conducting investigations at very low temperatures, he did not mean what he said; it was merely his way of saying that the vacuum receivers he was about to describe had not been made before. It was not I who made, it was Prof. Dewar himself who has made, a "glaring misrepresentation of the meaning" of his own words.

Prof. Dewar lays no stress, in his letter, on his own experiments on chemical action, phosphorescence, &c., at very low temperatures. It is these experiments, I suppose, that are referred to when he speaks of "throwing his bread upon the waters"; or does he abandon them as trifling, in that strange sentence—"Have I ever suggested that Prof. Olszewski was anticipated, or attempted to raise any question of priority?"?

Prof. Dewar hides the essential questions in a mist of words. If he has made marked improvements in the methods of liquefying and manipulating the more permanent gases (besides his invention of vacuum receivers); if he has conducted original, accurate, and thorough investigations into the properties of the liquefied gases, where are the accounts of this work to be found? Every student of the subject knows he can lay his finger on the work of Olszewski, and also on that of his deceased colleague Wroblewski; and he knows that work to be thorough, accurate, and important. But where can there be found some distinct account of the scientific work in connection with the liquefaction of the more permanent gases that is attributed to the Fullerian Professor at the Royal Institution?

I asked, and I still ask, for an "instant and serious consideration" of the whole matter brought forward in these letters by "those who are truly interested in the advance of science, and are jealous of the good name of the scientific men of this country."

M. M. PATTISON MUIR.

Cambridge, February 17.

Argon.

THE beautiful research of Lord Rayleigh and Prof. Ramsay has proved up to the hilt that in argon they have discovered an unknown gas which is remarkable for some of its physical properties, and especially for its extraordinary chemical inertness.

Among the interesting questions which have been raised, but not fully solved, are: Is argon an elementary body? If so, how does it stand related to the other elements?

There are some who advocate its being an allotropic form of a previously known element, just as ozone is allotropic oxygen; but it has not yet been produced from, or resolved into, nitrogen or any other element; and, until that should happen, it has the full right to take rank as a new element.

At first sight the fact of its giving two different line spectra under different circumstances might favour the idea of its being resolved into two bodies; but the fact that these two very compound line spectra have twenty-six lines in common, which are not all of them among the strongest lines, appears to me an argument for the fundamental unity of the substance.

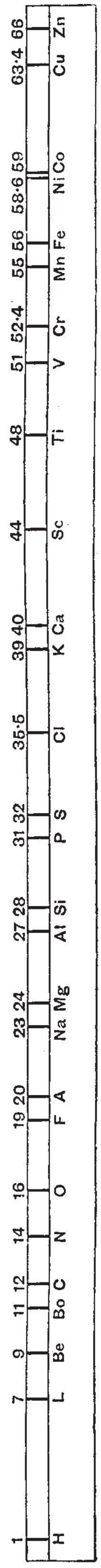
If argon be an element, what is its place in Mendeleéff's table? This is at present agitating the minds of chemists and physicists. The specific gravity of the gas would lead to an atomic weight of close upon 20; but it is argued from the velocity of sound through it that it is a monatomic gas, and therefore close upon 40. Now, if 20 be the atomic weight, it will fall in admirably with the periodic law; if 40, it will be perfectly inconsistent with it. The argument in favour of 20 is at least five-fold:—

(1) It will fill an existing vacancy in Mendeléeff's arrangement; that at the top of the eighth column. This is the first "even" series; and it should be remembered that the later "even" series in that last column are represented by the iron, palladium and platinum groups of metals; the "uneven" series being as yet unrepresented.

(2) It will follow the periodic law in regard to its melting point. That should be very low, like nitrogen, oxygen and fluorine; and so it is.

(3) It will follow the law in regard to its atomic volume. That is small; and so it should be by analogy.

(4) A great characteristic of argon is its not forming stable compounds with other bodies, at any rate at a temperature high above its boiling



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